REVIEWS.

ART. XXIII.—*On the Contagiousness of Tubercle.*

1. *Consumption as a Contagious Disease; with its Treatment according to the New Views. To which is prefixed a Translation of Professor Cohnheim's Pamphlet, "Die Tuberkulose vom Standpunkte der Infectionslehre."* By DANIEL HENRY CULLIMORE, Member of the King and Queen's College of Physicians. London: 8vo. pp. 124. Baillière, Tindall, and Cox. [No date. Preface dated, December, 1880.]
2. *Consumption: Is it Contagious?* By D. FRANCIS CONDIE, M.D., of Philadelphia. *American Journal of the Medical Sciences*, July, 1871, p. 119.
3. *Consumption: Is it Contagious?* By LAWSON TAIT, F.R.C.S. Eng., F.R.C.S., etc., Surgeon to the Birmingham and Midland Hospital for Women. *American Journal of the Medical Sciences*, October, 1871, p. 419.
4. *Is Phthisis Pulmonalis Contagious, and does it belong to the Zymotic Group?* By W. H. WEBB, M.D., of Philadelphia. *American Journal of the Medical Sciences*, April, 1878, p. 426.
5. *Is Consumption Contagious?* By EDGAR HOLDEN, M.D., Ph.D., President of the Medical Department of the Mutual Benefit Life Insurance Company, Newark, New Jersey. *American Journal of the Medical Sciences*, July, 1878, p. 145.

IF we are to accept the teaching of the authors, who have written the books and papers, the titles of which are given above, it is necessary to ascertain the meaning to be given to contagion. It would seem to be well —nay, essential that the boundary lines, as at present understood, between the things called contagious and those of an infectious nature should be altered. And it may be that the time has arrived when very properly some change of view or definition of these two conditions should be made. It is a shock to hear the word contagion used in the manner in which it is intended to be understood by these writers. In spite of the great confusion of meaning attached to these words and those of similar import, it will be found that, both in scientific works and in common parlance, there is a very radical distinction in the manner of communicating disease as implied by them. It is no longer possible to appeal to their definition, or to their use by the best authors. The two words are defined as synonyms, and yet in their use it is very easy to discover different shades of meaning or even very complete divergences, and that, too, in writers who at times make them synonymous. Contagion, according to its derivation, signifies contact, and yet in its common use contact of person is the farthest from necessary for the communication of disease. Contact, not of persons, but merely contact of a person with the material essence of the disease is the essential, according to the present acceptation of the word.

If we adhere to the definition of the word, syphilis, glanders, and hydrophobia are almost the only diseases requiring actual contact, direct or nearly direct, of person with person for their communication. Inoculation would seem to be a better word for expressing the more ordinary means of their communication.

Further, it must be borne in mind that in classifying disease under these various heads, it will be found their transmission is possible by more than one method. Variola is contagious, and is also inoculable. Syphilis is inoculable but not contagious in the way that variola is, viz., by transmission through the air.

But let us look at what have been considered as the factors of contagion. First, the development, not spontaneous, of a *something* within the organism which is possessed with the power of reproducing a similar train of phenomena in a second organism, and so on indefinitely. The chief peculiarity which attaches to contagious diseases is the means of their transmission from person to person, viz., that the way they travel and the *something* which travels are unseen. This latter peculiarity is, we think, the striking and prominent idea when the word contagion is used.

As we have said, syphilis and like diseases are, according to the etymological sense, the only necessarily contagious diseases because contact is essential to their transmission—other contagious diseases have other means for communication than contact—and these diseases are more conveniently named as inoculable ones. Syphilis resembles, however, the contagious diseases in possessing the *something* which indefinitely reproduces itself.

We have, therefore, the two classes, the one according to its ordinary method of transmission inoculable, the other class ordinarily communicated by unseen methods. This attempted classification is, we are abundantly conscious, not accurate, scientific, or logical, but neither is the word contagion, as at present employed, possessed of either of these three qualities.

Let us consider the question of infection and its relations to the two above described modes of communicating disease. This word is used by many writers as a synonym of contagion; perhaps it would be more accurate to state that the adjectives infectious and contagious are used interchangeably. We think, however, in every accurate meaning and valuable sense of the word that there is a very marked difference between contagion and infection. The etymology, which, of course, is not to be trusted very far when words have wandered so far from their meaning, implies a stained, or let us say, a poisoned condition, and this effect is usually applied to the air as the vehicle of transmission.

We think that this etymological sense adheres to the word infection and distinguishes it from contagion. The air may be infected by foul gases or other qualities by which diseased conditions are produced in most persons subjected to their influence, as is believed to be the case in yellow fever. Drinking water, as well as the air, may be infected and produce dysentery and typhoid fever. It is also possible to speak of the air becoming infected with the emanation from contagious diseases. But this is not saying much to the purpose, and at best it is not saying more than that the number of contagious-disease patients are very numerous, and the air very full of their emanations. Besides, it is confounding the two eminently characteristic methods of communicating disease, which we are endeavouring, almost vainly, to differentiate, because labouring with inaccurate words whose meanings overlap, or are partly inclusive of, each other. At best, to speak of the contagion infecting the air, is to describe the lesser by the greater. Variola and scarlet fever poisons, or emanations which are strictly contagious, may infect the air, but typhoid fever which infects water, is never contagious.

If then the words *contagion* and *contagious* could be limited in their signification to the transmission of the disease through air by impalpable or unseen means or agents, inoculation to those diseases where the means are palpable and generally gross, and infection to those morbid conditions which are not either contagious or inoculable, and for the production of which emanations carried directly from one organism to another were not necessary, we should be able to form distinctions between classes of disease that would appeal to a common idea of the general characteristics in respect to their ordinary modes of transmission more conveniently than is at present possible.

Unfortunately it is not possible to classify diseases according to this method; contagion and infection are words by which we can only hope to obtain a little help for characterizing certain qualities. By thus limiting these words we partially avoid giving expression to an hypothesis of the nature of the *something* which passes, and merely define the ordinary and most usual, but not the only, mode of passage. And this we consider to be a very desirable point to be gained, since, in these days of the germ theory and microbial organisms of disease, our ideas of the nature of a malady are so liable to be influenced by the theory of its transmission. The nature of a disease must be described, the mode of its communication may be characterized by a brief word. This criticism is applicable with especial force to the word *zymotic*, which has been used as synonymous with contagious or infectious; it is an attempt to connect the nature of the disease, viz., its supposed character of fermentation, directly with its mode of transmission, and to make the two separable conditions interchangeable words. The word virus, as well as the word poison, is likewise unfortunate. They are an imaginative use of words, implying facts and conditions of which we are still in ignorance, and when the knowledge is acquired in reference to them it will be much better expressed by other more definite terms.

In respect to the materiality of the *something* of disease, opinions have varied very greatly at different periods of medical history. At no period, we believe, has the essential immateriality of the matter been claimed, yet so closely has it been thought to approach an ethereal or spirituous character that it was quite beyond the appreciation of our senses, if not beyond the ken of our imagination. But in recent days nearly all things have become material, and while philosophy grudgingly allows the imponderableness of thought, no such tenuity is conceded to disease. We see, handle, plant, and cultivate, and harvest the contagious agent of many diseases, or think we do. The contagion-bearing fungi of measles and scarlet fever were pointed out some years ago, but have since been abandoned to the musty straw from which they sprang. Some years ago the ague-bearing plant was found in Ohio, and we saw it cultivated in Boston, and now another one, we believe of a different species altogether, has just been receiving great attention in Italy. Diphtheria too has its botany.

Now, while it is universally conceded that the *something*—the transmitting agent—of disease is material in all cases where transmission is claimed to occur, yet we think, in respect to the three modes of transmission, contagion, inoculation, and infection, the bulk or size, if not the quantity, of the material transmitted bears its share in qualifying the sense in which these words are used.

In contagion, impalpable and unseen, the disease is supposed to be carried by very small amounts of the material; the morbific agent, once brought in contact with the organism, unprotected from its effects, develops and reproduces itself, and is itself ready again for transmission. In inoculation, our natural ideas turn to larger bulks, something to be placed on our scalpels. The infective diseases call up the picture, if not necessarily of large amounts of the material essential to be brought in contact with the organism, at least of a large quantity of the poisoning material which is to flood the vehicle which carries it. Such ideas, respectively, we think necessarily attach themselves to the nature of the diseases and the modes of their transmission, so far as we are acquainted with their qualities, and do not merely appertain to the words selected to express the facts.

In respect to actual substance found in the giver and receiver—the originator and organism affected by the material of disease, our ideas ally themselves in accordance with the mode of transmission, although here, we think, the question becomes vague in proportion to our diminished knowledge of the nature of the material transmitted. In the case of inoculable diseases, and in inoculation experiments, we feel certain that portions of living matter are transferred from one person to another, not knowing whether this matter is cellular in character or a fluid. This transfer partakes of the nature of a graft, and to a certain extent the material continues its active life and growth, and it produces a distinct local effect. There is likewise the power of disseminating its action through the receiving organism to a greater or less extent, but whether this is done by the original received matter or from a new formation at the seat of introduction is of course unknown. Of those diseases which are confessedly both contagious and inoculable less is of course known; there may be two elements which have the capacity of transmitting the morbid process. The typically contagious diseases have the element of mystery, uncertainty, and impalpableness so intimately interwoven in our conceptions of them that heretofore no statement about the material transferred has been possible, and free scope has been given to such words as zymotic, virus, etc. That the transferred element forms a part of the organism, and is material and not merely an influence, may be predicated, although not as yet proved; this is probably the sum of our knowledge, and we think it accords with the opinions of men in general.

Numerous special investigations have recently advanced quite different theories. Those diseases, which we have designated as infectious, seem to be quite apart and alone in respect to the material agent which, when once brought in contact with an individual, is capable of producing the disturbance in the organism called disease. Both the other classes are regarded, and, so far as their nature is understood, are looked upon as distinct entities; by entities we would imply an actual continuation of the same morbid process, interrupted only so far as incident to the actual process of transfer from one organism to another necessitated. Quite different is the infection process. With infectious diseases, in the majority of them, the pre-existence of the material disease-producing agent in another organism is not necessary to our conception of the disease. Most authors believe that the material of typhoid fever, which is not contagious, is carried by infected water, etc., from one patient more or less directly to a second, but the general class of infectious, but not contagious, diseases are thought to exist potentially apart from their development in a patient. The man who ventures into the untrodden swamp is liable to suffer from dysentery and intermittent fever, but is not liable to the contagious scarlet fever unless it is carried thither by another.

This classification does not include all known diseases, but only those about which heretofore confusion has existed by the use of the words contagion, infection, and inoculation. We think the views here presented will assist in clearing up this confusion, and put us in a position to determine the question before us. We have said that the use of the word contagion as applied to the means of communication of tuberculosis was a shock to our preconceived notions of the matter, and that perhaps the boundary line between contagion and infection required to be shifted, not of course to meet this question, but to place it more nearly in accord with our increasing knowledge of the nature of disease in general.

One word further before considering the facts and opinions furnished by these authors. Contagious and inoculated diseases clearly imply an origin outside of the organism; infection is conceivable as having a source both outside and inside the person affected. Thereby the idea of infection is disassociated still farther from other means of communication and from other communicable diseases, and approaches more closely or shares a common ground with such morbid processes as are spoken of as constitutional diseases.

Having thus somewhat settled our minds by considering the basis on which the solution of this question rests, let us see what are the materials furnished by these writings.

The most important of these papers is the work of Cohnheim, his brochure entitled, "Die Tuberkulose vom Standpunkte der Infectionslehre." It is here given us in translation, and is prefixed to Mr. Cullimore's original matter on the subject "of consumption from a sanitary and pathological point of view, including its treatment according to the new views." In the translation, Cohnheim's pamphlet occupies twenty-four pages of the one hundred and twenty-four. We had the opportunity of reading Cohnheim's writing shortly after its first appearance in German, but do not have the copy at hand for comparison with its present form in English. We read the original with a great deal of interest, and have now re-read it with renewed pleasure, and each time we have been struck with the positiveness of the views expressed by him, which in this respect places the work in a striking contrast to others on this subject.

Cohnheim at first adverts to the opinions of Virchow (1860) on Tuberculosis, and especially in relation to the new doctrine promulgated by "Villemin's treatise (1865) on the transmission of tuberculosis by inoculation." He then alludes to the slow concurrence on the part of others, but "as time passed on, dissenting views respecting it decreased more and more, and now there are in the profession few who deny that *tuberculosis is a contagious disease*." We quote a portion of the sentence, with the italics of the author, as it strikes the key-note of Cohnheim's views of tuberculosis. He offers the "result of his investigations in the new field of inquiry" purely as a pathologist, and not from their clinical aspect.

The author describes first the distinction made by Virchow between true tubercle and the results of the hyperplastic and inflammatory processes; to the former belong the pin's point collections, which increase to nodules of varying sizes by the confluence of a greater or less number, and under the latter we think of the scrofulous lymphatic tumours and caseous pneumonia. These two products have nothing to do with each other in their genesis, but in their terminal stage suffer the common fate of caseation or degeneration. He next alludes to subsequent anatomical discoveries of the structure of tubercle, which have been brought into prominence since Virchow's original work—to the giant cells to whose frequency of appearance Langhans was the first to draw our attention. The work of others is also mentioned; for example, Weigert's criterion of tuberculosis, Schuppel's anatomical history of the lymphatic glands in this disease, and also that of other writers.

Cohnheim passes next to the gain medical science has won through the improvement in the method of making experiments, and records the fact that to these alone we owe the benefit of the discovery of the contagious tendency of tuberculosis. He speaks of the methods and the care required in the experiments of tuberculization, and it is to be regretted that so eminent and thorough an investigator has not here furnished us with more details of his work. This is, of course, impossible in the contracted space of his small pamphlet; but it is by these details only that his proposition can receive acceptance.

The conclusions from his labours are stated very definitely.

> "Tuberculosis can only be produced by tubercular matter. It is impossible to have found a more perfect criterion with respect to this particular disease. Every symptom tuberculosis has is manifested in animals experimented upon; those particular symptoms, and none other."

After very clearly setting forth the difficulties or the impossibilities of making an anatomical diagnosis between various kinds of deposits found in the lung, between the truly tubercular and those of inflammatory origin, he proposes to rely upon the inoculation test for their differentiation. Let us again quote his conclusion on the inoculation test.

> "If we inoculate with slaty indurations and peribronchial nodules, or with the thickened contents of a bronchial tube, the rabbit will not become tuberculous, a result which never fails if the inoculation be performed with tubercular virus. And what are we taught from inoculating with matter taken from human tubercle on one side, and those caseating, inflammatory, and hyperplastic processes? in short, scrofulous ones, or the other? Nothing more nor less than that all these products are potent in a similar degree"—"inoculation with a piece of lung affected with caseating pneumonia, of a caseating testicle, produces a like effect; and nothing succeeds better in inoculation than a freshly excised *scrofulous gland cut out of the neck.*"

He claims for the materials which produce the same results in experimentation a similar nature, viz., tubercular.

He goes on to show the unsatisfactory state of the microscopic proofs of the tubercular nature of deposits found in the organs, and with this statement every one must agree. He shows that the various criteria proposed are nearly valueless; the *kern*less accumulations of Lebert, the giant cells, and nodules which appear in their walls, are all conditions or appearances which are found in quite dissimilar growths or deposits.

His conclusion to this part of the subject is as follows:—

> "It is not caseation alone, nor kernless accumulation, not nodules or giant cells which are characteristic of tuberculosis, but *solely the caseation the result of specific causes, and the nodules which are derived from specific sources.* Let us try as we will, it won't help us; there are no anatomical definitions to be found for tubercle and tuberculosis. It must yield to the etiological." "Those who believe that all contagious virus is parasitical in its nature will not hesitate to believe that the tubercular poison is corpuscular; and we may await with certainty that in a not very distant future these specific corpuscular elements may

be demonstrated in the tubercular nodule and in the scrofulous products, which the lover of historical names may designate as the tubercular corpuscle."

We do not believe that Cohnheim's views will any more be productive of unanimity in relation to tubercle than those previously promulgated, and yet there is something very seductive and simple in his theory. But before we accept it, let us examine what his conclusions mean, and whether his argument is logically correct.

In the first place—and we think very properly—Cohnheim rejects caseation as the criterion of tuberculosis. This was the claim of Laennec's theory, that every caseating process in the lung was infiltrated or disseminated tuberculosis. Virchow rejected the claim. Now Cohnheim, while rejecting the criterion, reclaims all or nearly all the products of Laennec as tubercle, or tuberculoid, as well as many others which, until now, have never been so regarded, or only exceptionally so. He wishes to give them the name of "eruptive tubercularization." "The cheesy portions, as a rule, contain very little fat, and have the consistence of firmly coagulated white of egg." "It is not, however, necessary for us that they should be classed as tubercle; they are tuberculoid, and the softening and ulceration are only the direct consequences of the caseation." In other words, they are not tubercle, but tuberculoid, because his experiments with the material develop tuberculosis. He assumes in the first place that tuberculosis is a contagious disease, and then tries to prove that all these forms of matter found in organs are tubercle because they produce tuberculosis. The failure consists in the anatomical basis, both at the beginning and the end, being unknown or undefined. He neither defines the anatomical structure with which he starts, nor that at which he arrives or produces by the inoculation. It might perhaps help his argument, but of this we are not very certain, if occasionally, by inoculation with caseating pneumonia, he developed a caseating pneumonia, or to reverse the condition by using the round hard nodules, a caseating pneumonia resulted. But, no, whatever is used, the outcome is always the hard nodule. In this result he agrees with all other investigators, and we have seen the same ourselves.

He accounts for the result, not through the transplantation of any anatomical structure, but from the presence of a *virus*. "We can look upon caseating eruptive tuberculization and the little round nodules clustered in lymphatic cells as really belonging to tuberculosis when the matter inoculated from them produces tuberculosis; that is to say, when they themselves are the production of tubercular virus." This expression of opinion we regard as very unfortunate. We do have to speak of things as *virus*, and hitherto it has been confined in its use to explain conditions which were unknown, and as a convenient cloak to our ignorance. We have long spoken of the syphilitic virus, and the virus of a rattlesnake, but these expressions, if they mean anything, mean actual anatomical or similar entities, which we can handle, and which taken from known structure or morbid growths produce equally well-known anatomical results.

After speaking of the results of inoculation with a scrofulous gland, the author says: "We are consequently made aware of the close connection between these processes in spite of the difference of their anatomical genesis," and goes on to ask the question, "Would any one, because syphilitic caries is a different anatomical process from brain gummata and the eruption of psoriasis—would any one separate these things one from the other, and deny their connection one with the other?" We would answer, no; for the reason that these things can be shown to be the same by other methods than the gross external appearances. The bone gumma and the brain gumma agree in their histological characters, and these differ from each other and from the psoriasis or a syphilitic pustule only in so far as they are altered by the seat of their growth, or the tissue in which they develop. None of these are recognized to be of the same character, only by being derived from the same or similar initial lesion, or by producing the organic lesions of the same character when inoculated; their recognition depends on their histological characters. Cohnheim would not fail to recognize a bone or brain lesion due to syphilis, and would invariably differentiate them from tubercular disease. It seems to us weak to be prepared to class everything as tubercular, because, in the first place, no anatomical definition or delineation of the cells has as yet been found, and, in the second place, because various matters, both true tubercle, as acknowledged by all, and caseating material, thought by many or most others to be of different character, do, when inoculated, produce true tuberculosis.

It would seem much more justifiable to conclude that two things could by inoculation produce tubercle, since tubercle itself is, according to Cohnheim, not a definite anatomical entity, and can be produced by two other things equally indefinite. If tubercle were a morbid growth, such as carcinoma, this argument would not hold, and it would be difficult to escape from Cohnheim's conclusion. But we are dealing not with a definite anatomical entity, nor with a self-limited disease; it is rather a dyscrasia, and it is, therefore, not a difficult supposition that more than one factor or element may lead to its production. We think that an important fact has been overlooked in judging of this inoculation question which has not received the attention which it deserves. In all other inoculable diseases, living matter, or at least matter taken while in the living state, and most carefully preserved to be used fresh, is required, but in tubercular inoculations matter is used which, before the death of the patient, is dead anatomically, and has been so for indefinite periods before the post-mortem; even tissues preserved in alcohol are said to be efficient in communicating a living virus. Many suppositions can be made to account for the activity of this virus, but all require a greater faith than the plain straightforward statement that dead caseating matter, as well as fresh living tubercle, can reproduce tuberculosis in the inoculated subject.

It would seem as though the followers of this new doctrine had started out to find an anatomical entity, called tubercle, had failed to find it among the mazes which this dyscrasic change presents, and failing had recoiled into the old notions of Laennec, that every one of these processes was tubercular. This disease is not a morbid growth like carcinoma, it is not a self-limited or self-bounded disease like scarlet fever, and, therefore, we should not expect to find a particular cellular arrangement as in the one, or a contagion carrier, parasitic or otherwise, as in the other, for a disease like tubercle. We do not think we shall ever be gratified with seeing the "specific corpuscular elements," or the typical histological features of tubercle. In fact, we thought that almost every one had given up looking for specific cells in morbid growths, as well as in every other morbid change. We used to hear of cancer cells, but we never expected to be confronted with a *specific tubercle corpuscle*. We have rather passed beyond cells, since their form and size are dependent on so many purely accidental conditions, and reached the position where we regard all morbid changes from the manner in which they are born, and live, and die. And we try to think of disease as a perversion of normal growth, an excess or deficiency of physiological action, and not as some new entity living and growing in the body, and introduced from without. In accordance with this prevailing thought of modern pathology, it would seem much more likely that tubercle—since we can find no definite anatomical characteristics for the shapes and forms which it invariably assumes—is a perversion of growth rather than a distinct new formation, and we can find no other meaning in the inoculation theory, or a virus. We say we can find no other meaning in the inoculation theory, except that it is intended to imply a direct transference of anatomical elements, which reproduce similar anatomical forms in their new position; it is either this, or else that any caseating matters are productive of the dyscrasia called tubercle.

The second chapter of Cohnheim's will be found very interesting to all who are convinced of the contagiousness of tubercle, and his illustrations of the mode of invasion of the organism are very apt. We do not think, however, that many can repress a smile when they read that the virus of a contagious disease, in cases of tuberculosis, "passes from the tubes into the peritoneum, in genital tuberculosis in the female;" or "that the poison gets access to the brain through the nostrils, and passes thence through the foramina of the ethmoid bone." Have we gone back to the days of the Greek and Arab fathers in medicine that we have to support a theory by such suppositions and the aid of inoculation experiments with caseating material?

In respect to primary tuberculosis of the bones, here is the explanation advanced: "Although in most cases the development of tuberculosis has been traced to injury, yet it is not to be supposed that it can beget the tubercular poison; and so far as I can see, the only conclusion to be arrived at is that the virus is already circulating in the blood, and that the inflammation resulting from the injury attracts the poison from the bloodvessels and makes it settle in the tissues." This would be very good reasoning if the existence of a virus were shown conclusively, but it is not proof. We do not think the reason is as good as the old one, that the inflammatory matter, resulting from injury—perhaps caseating, for all we know—leads to the perversion of growth and the formation of tubercle. How unlike the virus of other contagious diseases, it does not appear equal to producing the disease in the organism in which it resides, but is all potential for inoculation. If the poison is circulating in the system, but unlocalized, a patient must be capable of communicating it, but does not show its manifestation until the bone injury "attracts the poison from the bloodvessels and makes it settle in the tissues." This seems to us very much like saying a person may disseminate a disease before he has it himself. We shall begin to think that the old Griefswald staff-surgeon of Cohnheim's student days, which the author quotes, was right, "We are all more or less tubercular."

In chapter third, Cohnheim speaks particularly of acute miliary tuberculosis, and the difficulty in accounting for these outbursts of the disease. He alludes to the theory of Buhl that the anatomical basis of tuberculosis is a pre-existing caseating deposit, which forms the starting-point of the acute eruption, and says that this is of no value to those who regard the disease as a contagious malady, since they consider caseous deposits "as a part of the tuberculosis and a production of the same virus." How is it with an inflammation at the appendix vermiformis resulting in an abscess and enlarged cheesy glands and subsequently development of miliary nodules throughout other organs? In these cases the miliary tubercle is most abundant in the peritoneum immediately surrounding the original disease. Does the author mean that the original inflammation was tuberculosis? Supposing the inflammation resulted from an injury, and it was only months or years subsequently that the enlarged glands of the part gave evidence of softening, and shortly after this event the eruption of miliary tubercle took place, do we need to introduce a supposed virus and a contagion to account for the result? In such cases, unlike the inoculation with caseous matter, the retrograde products of inflammation are introduced into the circulation by a process commencing and completed within the organism. In the inoculation experiments it is supposed that the caseating material contains a tubercular virus, but we think it is asking too much for us to believe that a perityphlitis is tubercular.

Local tuberculosis, which is also discussed in this chapter, does not favour a belief in the contagious theory, and the explanations offered by the author do not take away the difficulties in accepting this view. His comparison of tuberculosis to syphilis, as we have already said, is not a particularly happy one. Whatever may be the results of his experiments, he cannot mean to contend that syphilis is a contagious disease in the sense in which he thinks tuberculosis is contagious. No person would ever acquire syphilis by nursing or by breathing the same air as a syphilitic patient; no, not even if the patient had syphilitic disease of the lung. One does not acquire tuberculosis, nor syphilis, from living next house to a tubercular or syphilitic patient.

The whole question resolves itself into this, if the doctrine has any practical clinical value: that by such intimate relationship of two persons, the one nurse, the other patient suffering from tubercle, the healthy person receives into the lung, by inspiration, small particles of the tubercular matter detached from the diseased lung and expired by the patient. It is also claimed that besides the miliary deposits, other conditions of the lung, usually viewed as of inflammatory origin, have also the power of communication of the disease. In fact we believe it is not claimed that the miliary form of disease is contagious, but solely the latter stage of caseation. This power is supposed to depend on a virus of distinct and powerful kind, but tuberculosis, unlike every other contagious or communicable disease, does not, according to the terms of the new doctrine, ever produce such definite organic changes in the tissue which can from their histological structure be recognized and positively pronounced to be tubercle. As already quoted, Cohnheim says "there are no anatomical definitions to be found for tubercle and tuberculosis." Tubercle, therefore, is utterly dissimilar in this respect, according to Cohnheim, from every other known contagious or inoculable disease, at least of any of those about which a doubt of their contagious character can be entertained for a moment. Every other communicable disease has a definite anatomical lesion from which it takes its start, and is capable of reproducing the histological lesion. We do not believe that the independence, definiteness, and distinctness of a disease can be shown in fact, or maintained in theory, apart from its anatomical changes, gross or minute.

What is true of other diseases, we believe to be true of tubercle. We believe that tubercle has definite anatomical conditions, as definite as variola or syphilis. Whether tubercle can be communicated as typhoid fever is, or as variola, or whether it is communicable as syphilis is, Cohnheim has not shown. His inoculation experiments have not shown. They have merely shown that both from introducing miliary nodules and also caseating material into different organisms, free from tubercle, tuberculosis is developed. If he was content to claim that tubercle (miliary nodules) produced by inoculation tubercle, its inoculable nature could be granted—in fact, it must be acknowledged if he were willing to recognize the tubercle producing and the tubercle produced as definite structures. This he denies to them.

We think he denies it very unjustly. No one fails to recognize, both from its gross, as well as its microscopic appearances, a meningeal tubercle or one in the peritoneum, and each of these resembles the other quite as much as carcinoma mammæ does carcinoma of the liver. The great difficulty in this question of tubercle always has been, and appears to be equally so still with some, from the appearance presented by phthisical lungs. In the large or small cheesy masses, the infiltrated tubercle, so called, of Laennec, where nearly the whole area of the microscopic section presents a mere debris of lung tissues and granular and cellular elements, it is not to be expected that its original nature can be determined, but if tubercles are present anywhere in the lungs, there is usually enough lung or pleura not in a degenerated condition to afford samples of a fresh, well-defined tubercular growth or change. If tubercles are not found, the fair, common sense, straightforward conclusion to be drawn is that the disease is not tubercular. If they are present, it by no means proves that the cheesy masses are tubercular, or that they contain a tubercular virus, in the strict sense of this word. If the cheesy masses come first, the case is one of auto-inoculation, just as in the case of Cohnheim's and all other inoculation experiments. In both these latter supposed conditions, the presence in the body of a sufficient amount of caseating material leads to the development of tubercle. There is no need to suppose the presence of a virus or any contagion, unless we use the word in a loose popular sense; but these terms, applied in the manner suggested by the new doctrine, are strained, forced, and unnatural. Hence we say that our present meaning of contagion, and the boundaries between infection and contagion, stand very much in need of alteration before tuberculosis can be admitted among contagious diseases.

It cannot be contended for a moment that syphilis is a contagious disease in the sense which is intended by the new doctrine. Yet it is very possible to suppose that a fond and attentive nurse constantly bending over a patient suffering from syphilis of the lung might, by the inhalation of detached particles of the syphilitic disease, become syphilized; or if a large collection of material from chancres was made, pulverized and atomized for weeks and months in the presence of a number of people, possibly some of them would contract syphilis. If they did, the disease would have been derived from a definite anatomical lesion, and would reproduce one just as definite, both capable of diagnosis from definite histological character. If they did not contract syphilis, it is more than probable they would from bad air, etc., become affected with catarrhal pneumonia, which, in the course of time, would form cheesy masses in the lung. These cheesy masses by inoculation, so we are taught, produce tuberculosis in the lower animals. It would not be fair to conclude that syphilis in this roundabout manner leads to tuberculosis.

The remaining pages of the book are of original matter furnished by

Cullimore. After a few pages of preliminary remarks, which are quite interesting, but which in some of the conclusions drawn from them are not consequential, he passes first to the consideration of the histogenesis of the giant cell. The discussion of the giant cell in relation to tuberculosis has become very tiresome. We know of no sadder history than is furnished by a review of the various opinions on this subject. What a waste of valuable time has been made trying to read the nature of disease from the form of the cells instead of looking at the force which controls the activity of the component parts of the new formation! The author draws the correct inference in relation to this peculiar cell that its "presence or absence is of no particular importance, as helping us on the road to the discovery of the exact character of the tubercular virus;" but did he expect to see the virus in these cells?

He next speaks of the "test of tuberculosis founded on the results obtained by the introduction of the virus into the system." We have not yet alluded to the discrepancies and contradictory results obtained by different experimenters. Cohnheim is the only observer who asserts positively the invariableness of the result, but his translator seems to differ from him, and while acknowledging the contradiction, says they "do not affect the main question of the inoculability of the virus." It is evident how Cohnheim arrives at his positive assertion; he simply asserts that no material contains the virus that does not produce tuberculosis, and therefore using all sorts of material, the failures are called non-tubercular. Certainly such results should not be relied upon to prove the presence of a virus, especially when other observers, using material for inoculation which confessedly is not tubercular and can have no possible relation to it, such as bronchitic sputa and cheese, are able to produce a tubercular deposit. And when it comes to explaining these discrepancies by supposing, on the one hand, that the inoculated subject has already acquired the disease by previous contagion, and that the virus circulated in their vessels, and under the influence of the inoculation the virus left the vessels and settled in some organ; or, on the other hand, that the person from whom the bronchitic sputa was taken was subject to *latent* tuberculosis; with suppositions similar to these before us it becomes impossible to argue. Was the milk from which the cheese was made taken from a supposed tubercular cow which had as yet no manifested local disease?

At one time the supporters of the new doctrine argue for a small amount of virus in the blood, at another for a very large amount; it would seem, therefore, probable that a number of observations could be accumulated where the blood-containing virus was capable of producing tuberculosis by inoculation. However, we know of no such observations. In this chapter we next find considered some minor objection to contagious virus, and the question of food as a vehicle of contagion. all of which is well given, but which does not add any proof to the contagiousness of the disease.

The remaining portions of the work are devoted to the symptoms and etiology of consumption, and of acute miliary tuberculosis, and the prognosis in accordance with the teaching of the new doctrine. The third and fourth chapters refer to the question of treatment, especially in reference to climate, etc. etc., and the immunity of certain countries as compared with others from the disease. An appendix is given consisting of meteorological tables.

The other articles on the subject of the contagiousness of tubercle or consumption, the titles of which head this review, have so recently appeared in this Journal, that it is not necessary to give any abstract of their contents. They all consider the subject from its clinical aspect, and adduce the proof from the opinions of practical physicians. Dr. Holden is particular to state that the question proposed is whether consumption is, without direct inoculation, communicable by one person to another, and Dr. Webb considers its inclusion in the zymotic group of diseases.

Dr. Holden states that "the real point being whether any septic material is eliminated from the body of a person suffering from necrobiotic changes in the lung, which, floating in the air or transferred by contact, may develop the same affection in another person," but we conceive that the question is something really very different in settling the question of contagiousness. The question which he states is a most important one, and no other one can possess a more vital interest so long as the mortality lists show so high a death-rate from this disease.

Let us see what Dr. Holden's point implies in relation to this question of contagiousness. His question is of *any* septic material floating in the air producing the same disease. What does he mean by "any septic material" and by "the same disease." The "any" must be identical with the "same" in order to show contagion. Smallpox is produced by septic material floating in the air, but the septic material is the same, and is—according to universal belief—derived from and identical with that derived from a previous case of smallpox. To prove the sameness of the septic material with the disease, namely, tubercle, inoculation-experiments have been appealed to. What is their answer? That tubercle can be developed by the introduction of true tubercle nodules, by caseating material, and by cheese.

We are ready to grant that any septic material—or almost any—does produce, when properly introduced, tubercle. We know of no experiments which show that it, viz., caseating material, produces anything else but the small miliary nodules; the experiments, we believe, have not been claimed as showing that they can develop catarrhal pneumonia or other necrobiotic lung changes, and it is very certain that such changes occur in animals not inoculated but housed in a similar manner to those subjected to the experiments. We do not think, therefore, that the essential sameness has been shown. We do not wish to cavil with words, but suppose any one should say that bricks, from a falling building, flying through the air came in contact with a person's head and produced a fracture of skull, and any one else showed that the fall of decaying trees in a forest did the same: the necrobiotic changes are evident in both cases, and the fractured skull is the same; yet there is a want of sameness between cause and effect. It is not claimed that fractured skulls are contagious, although there is a very evident diseased condition perceptible in the skull, and this condition was brought about by a diseased condition of the house and of the tree. The want of likeness between the diseased thing producing and the diseased thing produced is in this illustration very evident, and want of likeness is to us equally evident in the destruction of the lung, called phthisis. In the first place, no one believes that all cases of consumption are due to contagion in the sense intended by the new doctrine. Secondly, it can be shown that the conditions anatomically viewed are not identical, and Cohnheim, as already quoted, expressly denies any definite anatomical criterion. In all other contagious diseases identity of anatomical forms is very clear, for example, variola; in other cases where no very marked anatomical change can be found post-mortem, the symptoms, course, results, etc., are so definitely uniform, and so accurately reproduced, that there is no longer any question of identity. How different is it with phthisis!

Among the later papers which have appeared on this subject, we wish to call attention to one by Dr. Whitney on "The Inoculability of Tuberculosis," which is to be found in the *Boston Medical and Surgical Journal* for July 28, 1881. It is an exceedingly clever paper, and covers very completely the pathology and bibliography of the subject from the beginning to the present date. He considers the three-fold aspects of the question, viz., the inoculation experiments, the food, and from inhalations.

It is with the scientific aspect of the question that we have mostly dealt, rather than the two latter and more practical and every day points, and we believe that this is the proper point from which the subject should be viewed.

Unquestionably the subject from the first point of view is not by any means a settled one; in fact, whether it will ever be settled with unanimity is, we think, doubtful. Some one will be found, as has continually been the case during the past half century, who is ready to introduce some immature but plausible supposition to disturb the professional mind.

What we think is more important to settle than the absolute correctness of the inoculation experiments, is what we intend to prove by them, granting their correctness. And in considering the contagiousness of tuberculosis, we want to settle if we mean that infectiousness is the same thing as contagiousness. It does not make a very great difference whether it is or is not; but settled definitely one way or the other it must be. And next, are we to consider the question proved for or against contagiousness (or infectiousness), if a varying lot of matters produce by inoculation tubercle. It seems to us that success with one small piece of cheese in producing tubercle spoils the whole attempt at proof. There is no longer a like producing a like; it is various unlikes always followed by the same like. It would be in some sort a confirmation of Cohnheim's peculiar view if occasionally the various unlike substances used in inoculation were followed by the production of unlike results—say, for example, a miliary nodule caused a caseous pneumonia, or if cheese at one time caused miliary nodules, and at another catarrhal pneumonia.

Whatever objections are offered to the inoculation experiments, or however much doubt may be thrown on their evidence, we cannot question the importance of the dangers involved to individuals of becoming tubercular by means of food derived from tubercular animals, and by the inhalation of breath or of particles expelled from the lungs of tubercular patients. It is claimed that the disease is communicated by this means, and it may be that it is more frequently communicated than we are in general aware. The evidence of it in respect to food is not very abundant as yet, and in respect to inhalation, it partakes of that general nature which does not rise above the grade of possibility, or even, let us say, probability. It must be remembered, however, that the communication of the disease by both these means is comparatively a new investigation, difficult to carry out with exactness, and from which in time more positive data will become available

The discussion of the communicability of the disease by means of food, especially to infants and children by milk, seems a much simpler and easier question than that by inhalation or direct contagion or communicability from person to person, because with animals the carcasses and the pathological alterations are ever ready to our hand for examination, and it can at once be settled whether the milk-furnishing animal is the bearer of a true tuberculosis, or is afflicted with a catarrhal pneumonia or some other pathological process. Let us be careful to bear in mind that because the milk from a *sick* cow is fed to an infant, who subsequently becomes tubercular, that it is not a proof that tuberculosis is contagious.

The same criticism must be applied to the communication of this disease from patient to nurse. It would seem, from the records furnished by at least two of these papers of which we have spoken, that both at present and in past times, a very considerable number of men have viewed with awe and dreaded the possibility of a consumptive communicating the disease to healthy persons who were in contact with them. But how much does this opinion prove?

With every desire to believe the evidence in a matter of so great importance, we must still insist on the great difficulty there is in showing the true sequence of cause and effect; in other words, to determine with accuracy when the *post hoc* becomes a true *propter hoc*. If contagion or infection from external sources were regarded as the sole cause of tuberculosis, the decision of the question would immediately become a comparatively simple one; but the most enthusiastic advocate of the contagion theory does not claim that this is the only means of its communication, and fully recognizes that many cases of tuberculosis originate *de novo*, and by what we may call self-infection or self-inoculation.

So long, therefore, as the double source of origination is spoken of, the results of contagion would always necessarily be regarded as most doubtful of belief. Granting for the sake of the argument that a patient suffering from a destructive disease of the lung does cause, as the result of close contact, a similar condition of the lung in a healthy person, what is proved by it?

Very certainly that the two persons ought to have kept farther apart. But does it prove contagiousness? No; and the inoculation experiments answer the question also in the negative, by showing that almost any foreign matter introduced into the organism is capable of produing genuine tubercle and tuberculosis. It is strange that the phthisical patient never communicates tubercular meningitis or peritonitis to the nurse, or that general tuberculosis, of other organs than the lung—conditions, therefore, in which the blood must be surcharged with *virus*, if any exists—does not propagate similar diseased conditions.

The natural objection which the advocate of the contagion theory may interpose in cases where the diseased spot does not obtain free communication with the channel of its transportation, as in lung disease, is quite unreasonable. A disease which is contagious, and has a virus, is certainly a blood disease, *i. e.*, the virus itself must be circulating in the blood, and therefore carried freely to the air by means of the lung, as well as by other secretory and excrementory channels. To deny this, and to claim that tubercle is only contagious when seated in the lung, is at once to dethrone it from its position as a contagious disease, and to make it a purely local matter. To claim that it is communicable only when seated in the lung, introduces the doubtful element of the possibility that the air from the breaking-down lung carried, not a virus, but just such matters as we know from experiment are capable of inducing tuberculosis,—matters which are themselves not tubercle, but merely disorganized products.

Again, granting that a person suffering from destructive disease of the lung does, by means of inhalations or other contact, induce genuine tubercle in another, what is it necessary to show before contagion is proved? In the *first* place, that the disease in the contagion-giver was identical with that of the receiver. It ought to be shown that the two have a closer resemblance than merely the power to destroy the lung tissues. To show the lung broken down in one case and genuine tubercle in the other does not prove the *contagiousness* of tuberculosis. No one except those who advocate the contagion theory disbelieves in an anatomical structure by which tubercle can be recognized. Cohnheim apparently disbelieves in an anatomical structure because matters other than those universally recognized as tubercular do, when inoculated, show a power of producing true tubercle. This argument is very much like a circle, and then finally begging the question. *Secondly.* To prove contagiousness, it must be shown that disease, as occurring in the receiver, was not an accidental catarrhal inflammation, which has, as we know, a power of auto-inoculation.

Finally, the only other case to be supposed is that of a nurse acquiring catarrhal pneumonia from contact with a chronic catarrhal pneumonia patient. We believe no one claims that catarrhal pneumonia is contagious.

M. L.

NOTE.—The following are the latest reported investigations of the subject of the communicability of tuberculosis:—

M. Toussaint reported to the Academy of Sciences in August last on the results of his experiments in producing tuberculosis with lymph taken from "a cow in an advanced stage of tuberculosis." Toussaint's account mostly relates to the existence of a tuberculous microbion and to its artificial cultivation.

Krishaber and Dieulafoy have been repeating the usual inoculation experiments with monkeys "as being the animals which the nearest approach to man," and show a very large proportion of successful results in producing tubercular lesions. These observers claim that the "tubercular granulations prove most rapidly transmissible, while the pulmonary parenchyma was less infectant." Their communication was addressed to the Académie de Médecine last August.

ART. XXIV.—*Die Actinomykose des Menschen, eine neue Infectionskrankheit auf vergleichend-pathologischer und experimenteller Grundlage geschildert.* Von Dr. E. PONFICK. Mit 6 Tafeln. Berlin, 1882: pp. 132.

Human Actinomycosis. The Description of a new Infective Disease based upon Comparative Pathological and Experimental Study. By Dr. E. PONFICK. 6 plates. Berlin, 1882: pp. 132.

AT the recent twenty-fifth anniversary of Virchow's professional residence at Berlin, many of his distinguished pupils and assistants gathered together for the purpose of paying him their respects and congratulations. They then presented to him renewed proofs of their scientific and intellectual activity in the form of original articles relating to some subject in medicine towards which their attention had been directed in consequence